It is becoming more generally recognised that the really "practical" man is the one who combines practical knowledge and experience with intelligent appreciation of underlying principles.

The aim of the book is to lay before sailors, in an easily comprehended manner, the principles on which the various mechanical devices employed by them are founded.

A large amount of useful information has therefore been gathered together in the small compass of this book, and rules and principles whose *general* application is explained in various text-books on practical and applied mechanics, are here specially adapted to the requirements of the sailor. To mention a few instances: we find explanations of the mechanical advantage gained by the various tackles and purchases; the construction of derricks and shears, and the weight they will support; the relative strengths of ropes; the breaking strains of spars; the floating power of spars and casks, and the weight which a raft, constructed of given materials, will bear; the effect of the wind on the sails in driving the ship ahead and in causing leeway; the effect of the water on the rudder; the extra strain thrown on slings when a ship is rolling, &c.

The principles of the composition and resolution of forces, and of the mechanical powers are somewhat fully explained in the opening chapters, and the idea of applying the traverse table, so familiar to all sailors, to the solution of the problems, is an excellent one; but more explanation of some of the rules given later, which have to be taken for granted, could be desired, as it is very difficult to retain bare rules in the memory.

The size of the book no doubt imposed limits on the amount of space to be devoted to explanation, but it provides, nevertheless, an excellent book of reference; and though it may not be necessary to make some of the calculations referred to, it is always useful to know how things are worked, and on what principles, and that the principles are in accordance with well-known physical laws. "A sailor is so often thrown on his own resources, and the more exact his knowledge is of natural forces, the more readily can he avail himself of the forces at hand."

The book contains several valuable tables, and a useful collection of rules in mensuration.

F. C. STEBBING.

Power Locomotion on the Highway. By Rhys Jenkins, M.I.Mech.E. Pp. 64. (London: William Cate, Ltd., 1896.)

THE sub-title of this publication sufficiently expresses the character of the contents; it is "a guide to the literature relating to traction engines and steam rollers and to the propulsion of common road carriages and velocipedes by steam and other mechanical power, with a brief historical sketch." The historical sketch is a concise statement of the lines along which progress in power locomotion on common roads has proceeded. Following it is a bibliography of works on mechanical carriages and traction engines, a catalogue of papers read before, or appearing in the Transactions of, scientific and technical societies, indexed under names of authors, a list of journals devoted to the mechanical carriage movement, and an index to articles on the subject in periodical literature up to the end of 1895. The periodicals indexed include those of the United States, France, and Germany, as well as of Great Britain. The author has evidently been at considerable pains to prepare his descriptive index, and his efforts deserve encouragement. It would be an immense boon if indexes of the same description were available for other branches of technology. The reception afforded to this little book will show whether the demand is sufficient to justify the publication of others of a like kind.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Utility of Specific Characters.

I REGRET very much that I did not correctly remember, when writing to NATURE a month ago, what my friend Mr. Thiselton-Dyer had said at the Linnean Society's meeting. I suppose that in consequence I must not greatly complain that whilst telling us what he really did say, my friend has taken the opportunity to present a version of my views which is far from accurate. He has less excuse than I had in attempting to cite his remarks, since he has before him my printed letter of July 16. This fact also renders it easy to show wherein he is inaccurate.

I suppose that we are all agreed that it is in the highest degree interesting to know what Mr. Darwin himself thought and said on questions of the kind now under discussion. At the same time, we are none of us, I imagine, likely to attribute to Mr. Dyer a special knowledge either of Darwin's writings or of their interpretation which we do not share. Naturalists are, I believe, not prepared to accept any individual as the authoritative exponent of Mr. Darwin's teaching. Under these circumstances it is to me a matter for regret that a plain discussion of the question whether specific characters are invariably useful should be turned into a dispute as to whether the person who suggests some special application of Mr. Darwin's doctrines, or advances some subordinate hypothesis in relation to their application, can or can not be solemnly regarded as an Orthodox Darwinian. Mr. Dyer says that the Darwinian theory seems hardly to have a convinced supporter left except Mr. Wallace. He denounces my quotations from Mr. Darwin's own books as to correlation of variation as a "difficulty" brought up by me "against" the Darwinian theory; then, without more ado, assumes the *rôle* of apostle of the Darwinian theory (a part to which I cannot allow him any exclusive claim), and proceeds to tell us what "will" be found in the twenty-fifth chapter of Mr. Darwin's "Animals and Plants under Domestication," viz. that Darwin has said pretty much all that can (as yet) be said about the facts of correlation of variation. The attempt on Mr. Dyer's part to represent my citation of Mr. Darwin's own conclusions in regard to correlation of variation as unorthodox, is a little beside the mark. No one is ignorant that it "will" be found that in the chapter cited Mr. Darwin discusses "correlation"; not only that, but it *has been* so found long ago and repeatedly by many other readers besides Mr. Dyer. I expressly stated, both at the Linnean Society and in my letter printed in NATURE on July 16, that I was quoting from Mr. Darwin both as to Wells and as to other instances.

Perhaps the most unsatisfactory feature in Mr. Dyer's claim to classify his friends as heterodox and orthodox in regard to Darwinism, is that it leads him to undertake to give away the Darwinian theory. "I frankly admit," he says, "that such a case [a case of correlated variation such as that hypothetically stated by me] if completely established would give the utility of specific characters, and with it the Darwinian theory, a serious blow." I do not value this frank admission. If Mr. Dyer feels constrained to admit to some one that such a case would give a serious blow to the Darwinian theory, he must not come to me with his "admission" of a "point scored"; for I neither admit that any such blow is given, nor can I accept Mr. Dyer's good-natured offer to act as representative of the Darwinian theory. All that my friend can represent in this matter is the Dyerian theory of Mr. Darwin's theory. Mr. Darwin never asserted in so many words that specific characters are invariably "useful," and in my judgment he did not hold that opinion. But whether he did or did not, that opinion can not, I think, be shown to be a necessary outcome of the theory of natural selection, provided that we take into consideration important ascertained properties of living matter. My impression is that Mr. Wallace—whom Mr. Dyer has declared to be the only convinced supporter of the Darwinian theory now left—stated at the Linnean Society that he agreed with me as to "correlated variation" sometimes accounting for a specific character which accordingly could not be regarded as due to utility. Such also I gathered was the view of Mr. Meldola. Yet neither of these gentlemen regarded this conclusion as a serious blow to the Darwinian

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theory. Nor do I; on the contrary, I regard it as an important aid to the general application of the Darwinian theory.

Apart from the objection, which I have above expressed, to the treatment of this question by Mr. Dyer as one of orthodoxy, I note with regret that he has (unintentionally no doubt) misrepresented what I have actually said in my letter. Whereas I have expressly cited Mr. Darwin's principle of "correlation of variations," Mr. Dyer writes of the "extended correlation principle of Prof. Lankester." This form of pleasantry could be itself extended, but more of it would be unworthy of your pages. As a matter of fact, what I said as to correlation was very little more than a citation of cases and their theoretical explanation given by Mr. Darwin. To represent this as in any way parallel to the independent and anti-Darwinian theories of Mr. Romanes and Mr. Bateson, as is done by Mr. Dyer, is misrepresentation. The theory of Wells as to the black races of the tropics was used by me in order to illustrate my suggestion as to correlation leading to a development of useless specific characters. I might, equally well for my purpose, have used any of the other cases collected by Mr. Darwin. Whilst I spoke of Wells' case as "striking and suggestive," I at the same time expressly referred to it as "a more or less hypothetical case." Mr. Dyer has no justification, so far as I can see, for stating that I use this case as a foundation for wide generalisation. I made no wide generalisation, but adduced the wide generalisation at which Mr. Darwin, after collecting and considering a large variety of cases, arrived, viz. that correlation of variation does occur largely and generally in the organic world. This wide generalisation is, I say, not mine; it is Mr. Darwin's. If this generalisation be accepted—and we may reasonably hope that the apostolic and orthodox do accept it—then it seems to me in the highest degree probable that an obscure specific difference of structure—highly effective as life-preserving or progeny-ensuring—will more or less frequently carry with it as a correlated variation a more obvious and measurable character in some remote part of the body, not effective, that is to say, not useful. Hence I conclude that there may be specific characters (not by any means all or always) which are not themselves *useful*, though readily observed. That is the whole of my contribution to the present discussion. It does not seem to me to involve anything rash or surprising, though such is its character according to Mr. Dyer.

I may remind the readers of NATURE that some years ago, in these columns, I adduced this same principle, viz. Mr. Darwin's principle of correlation of variations, as one sufficient to remove some of the difficulties in the way of the doctrine of natural selection brought forward by the Duke of Argyll. What the Duke called "prophetic germs" might, it seemed to me (and still seems to me), when not explicable as lapsed rudimentary structures, be accounted for as variations or new structures correlated with other useful and therefore selected variations, although not yet themselves useful. A useless variation correlated with a useful one must (it seems to me) be supposed to pass through initial stages in which it is too small, or otherwise insignificant, to be useful (its utility or harmfulness being swamped in the utility of the correlated useful character), and only after attaining considerable development becomes either useful or harmful, and therefore subject to selection, possibly under some slight change of environment.

I regret all the more my differences with my friend Mr. Dyer, because the explanations they have involved leave me so little space in which to refer to Prof. Weldon's courteous and interesting letter. I have only one point to correct in his statement of my position in relation to that taken by him. The attempt to reconcile the *dicta* of Hume or Kant, or even of Mill, with the experience and approved practice of those who make it their business to investigate natural phenomena, would be an interesting undertaking, possibly one beyond the powers of living man. It is certainly not one upon which I shall here and now embark.

The point whereon Prof. Weldon has misunderstood my contention is this. After describing a phenomenon (death-rate) preceded invariably by two or more phenomena of structure or function, he says: "Under these circumstances, Prof. Lankester thinks it legitimate to pick out one of these antecedent phenomena and to speak of it as the only effective cause of change in death-rate, the other phenomena, although equally universal, being merely unimportant concomitants of this one essential change." Prof. Weldon is mistaken in stating that I think it legitimate "to pick out" without qualification, or at haphazard, any one of these antecedent phenomena, and to speak of it as the only effective cause. I should object to such a proceeding on much the same ground as that on which I object to his calling "any and all" of those antecedent phenomena effective causes.

What I think is the reasonable course in such a case—supposing that a man wishes to ascertain, as fully as may be, the relation of these phenomena to one another, is that he should frame in his mind a hypothesis as to how any one or more of the phenomena, invariably associated with a given death-rate, can operate so as to effect an increase or decrease in that death-rate. This, no doubt, will require a large knowledge of the surrounding conditions not usually to be acquired with ease, and an analysis of the antecedent phenomena in question, often of a prolonged and laborious character. Sometimes, however, such a hypothesis will present itself very readily and with much antecedent probability. However attained, the hypothesis will remain merely a guess until it is tested. It can be tested either by experiment or by observation of appropriate natural instances. By repeated testing, involving often great ingenuity and prolonged labour, the hypothesis is either confirmed or discarded; possibly a new hypothesis is adopted, so to speak, *en route*, and established as in all probability true. When—and not until—this process has been gone through, the naturalist will be, more or less according to the extent of his work, in a position to *place* the phenomena in their true relation to one another and to the ultimate phenomenon proposed for investigation, viz. death-rate. If the study of the antecedent or associated phenomena by means of hypothesis and test-experiment has not been and cannot be carried out, the naturalist can not (it seems to me) reasonably either "pick out" one of them and assert that it is the cause of increased or decreased death-rate, nor (still less) declare that all the antecedent or associated phenomena are the causes and produce the effect of increased or decreased death-rate. If he does do so, he appears to me to be evading the task before him, which is to "explain," that is, to place in their true order and relation a complex group of related phenomena.

The appeal to analogy no doubt frequently leads naturalists to rapid conclusions as to the causal relations of the phenomena of organisms. Often (but by no means always) such conclusions are erroneous: but it is not on that account desirable to reject the argument from analogy in reasoning about such matters. It must be used with knowledge and with caution. It seems to me, that in considering a complex case in which actual experiment is as yet wanting, it is often more useful to formulate provisional conclusions as to what is cause and what only concomitant effect by the aid of argument from analogy, than to deliberately reject all attempts at analysis, and to "lump" all the constantly associated phenomena as "causes." Surely in the case of Prof. Weldon's crabs, most naturalists would take the view that the frontal measurements may *possibly* be operative in saving the life of the crab, or *may* be only a correlative of some other life-preserving structure; that its quality in this respect should be inquired into by means of hypothesis and experiment; and that, until this is done, it is *premature* to speak of a particular frontal proportion as having for its *effect* the survival of those crabs distinguished by its possession.

The chief task of the student of living things seems to me to lie in the search for such explanations, even though the task is in some cases to all appearance at first sight hopeless, and even though too hopeful and imaginative spirits may be led, not unfrequently, to propound explanations which are insufficiently supported by observed facts, or are demolished by the observation of other facts. You can not (it seems to me) reduce natural history, as Prof. Weldon proposes, to an unimaginative statistical form, without either ignoring or abandoning its most interesting problems, and at the same time refusing to employ the universal method by which mankind has gained new knowledge of the phenomena of nature—that, namely, of imaginative hypothesis and consequent experiment. I think that most naturalists will agree with Johannes Müller that "Die Phantasie ist ein unentbehrliches Gut." E. RAY LANKESTER.

Dinard, Bretagne, August 10.

Habits and Distribution of Galeodes.

WILL you be good enough to publish in your widely-read paper the following notes on the geographical distribution of *Solfuga* (*Galeodes*) *araneoides*, as the published accounts of that arachnid are incomplete in that respect.

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